Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2022-506-RC1, 2022

Comment on acp-2022-506

Anonymous Referee #1

Referee comment on "Using Orbiting Carbon Observatory-2 (OCO-2) column CO_2 retrievals to rapidly detect and estimate biospheric surface carbon flux anomalies" by Andrew F. Feldman et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-506-RC1, 2022

Feldman et al. present an analysis of the ability to use OCO-2 XCO2 observations to detect and estimate biospheric surface CO2 flux anomalies over the Western US using a simple mass balance approach. They find that in a synthetic testbed scenario using CarbonTracker estimates and a large enough domain to reduce the inflow of background CO2 concentrations the simple mass balance approach is capable of detecting monthly surface CO2 flux anomalies. However, in a real world scenario with OCO-2 XCO2 observations this method is only capable of detecting large surface anomaly enhancements and only when the OCO-2 XCO2 anomaly enhancements are above the 90th percentile.

This is a well written and structured manuscript exploring an interesting and alternative (to atmospheric transport inversions) application of the OCO-2 XCO2 observation. The readability and scientific credibility of the manuscript will benefit from a few clarifications by the authors.

- What constitutes the XCO2 retrieval noise level (mentioned in line 74), does that also include both systematic and random errors? Later on (lines 472ff), the authors argue that spatial autocorrelation of errors does not change their derived error standard deviation when relaxing the assumption of independent errors. This is not clear to me; there should be a difference of 1/sqrt(n), with n being the number of averaged observations, between assuming fully correlated errors and independent errors. Further, the authors mention compensating effects due to subtracting two anomaly error estimates (Western US XCO2 anomaly error minus Pacific Ocean XCO2 error anomaly), but this should rather increase the error of the difference.
- The authors do consider the effect of advection of CO2 from background regions perturbing the signal in the XCO2 observations but they neglect the impact of inflow of CO2 to a total column estimate from atmospheric layers above the boundary layer. The study would be strengthened if the authors could show that this is negligible.
- How is the analysis impacted by the choice of region, especially since there has been an ‘ongoing decadal-scale megadrought’ and the XCO2 climatology only consists of less than a decade?

- What are the limiting factors for the selection of the domain? Or in other words which region characteristics influence the anomaly detection most: topography (and hence advection), heterogeneity in land cover, human footprint on the emissions in the domain, ….?

Some additional points:

L 90: Please add 'CO2' here: … can be used for surface CO2 fluxes - …

L 164/165: If the LPJ annual fire emissions and the annual sum of the QFED biomass burning emissions are not of the same size, this then effects the carbon closure in LPJ, i.e. the model would not be mass conserving anymore and LPJ would simulate more/less heterotrophic respiration in the following year (depending on the sign of the difference). I doubt that this effect changes the analysis in the manuscript but it is worth mentioning.

L184: Spatially averaged to which resolution?

L 354/355: add 'be': … during the summer months may be the cause…

L 635: Should it be 'Y.Y. provided GPP…"?

Fig 1: It would be nice to see each month individually and not seasonally averaged, at least as a supplemental figure.

Fig 4: How large are the errors in relative terms?

Fig 6: The stipples are not clearly visible, please revise such that it becomes clearer which gridcells show significant correlations.